Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

THE PHYSIOLOGIC ASPECT OF THE SPECIES QUESTION

PROFESSOR J. C. ARTHUR

PURDUE UNIVERSITY

NOT being sure of what ground might already be covered by the speakers who have preceded me on the program, I take the liberty of presenting my matter after giving thought to what has been said; and I find in mentally reviewing this—that the species concept as presented by the previous speakers does not embrace the whole field as it is forced upon me in my every-day work. I am especially surprised at this, because the first speaker, under whom I secured my elementary training in botany, is very particular in giving instruction to his pupils to begin with the one-cell plant and to work up from the simple to the complex. Yet, so far as I could ascertain in listening most carefully, he has not now taken into consideration a single one of the simple plants of which he knows there are many thousand times as many as there are of flowering plants, therefore I seem to be left somewhat adrift for my presentation.

I will not attempt to give any historical aspect of the question, but to take it up from the point in which it appeals to me in my every-day work, for it is a problem that I have been obliged to meet, and as I have met it, I will present the results to you.

As I understand it, we are taking up the question of the species concept. I thought it quite well worth while, in looking up the matter, to see what a man who has entirely changed the aspect of the subject within not so very many years might say as to what he considered a species concept, and so I very carefully again looked through my copy of Darwin's "Origin of Species." If a man were going to revolutionize the world of thought he certainly, I assumed, would give a definition of the subject he is going to treat. But I could not find a word as to what Darwin meant by species. He writes as if all the world knew what species are. Yet I did find this in the latter part of the last chapter; he says, "And now we shall be freed from the vain search for the undiscovered and undiscoverable essence of the term species." Consequently, here we are, tracing a will-o'-the-wisp. And yet, it seems to me, there must be something that we are all thinking about when we say species. There must be some workable idea. Of course I know that since Darwin's time and the vast accumulation of knowledge in reference to heredity and variation, to say nothing about other physiological aspects proper, our ideas have decidedly changed. Still, as I look over the actual work being accomplished in determining the number and the extent of the species of plants in the world, I do not see that our practise especially differs from the pre-Darwinian practise. We have a theoretical idea of what species are, but practically we describe them for the most part in the same pre-Darwinian terms, the really Linnean terms, so far as many of us can, although we do not adhere to the Linnean brevity. Nevertheless, there are some instances in which it seems necessary to deviate from this practise, if we wish to handle our subject in such a way as to make it useful; for I assume, in the first place, that whatever the species concept may be, it is something which enables us to present ideas or problems in a better form than we could otherwise accomplish. At any rate, the idea of a species is a mental tool of some sort.

To take up the purely physiological aspect; possibly it was the study of bacteria which forced this phase of the question upon us for the first time. I judge that it came largely from the fact that bacteria are so very minute. What could one do if he were going to describe the thousands of species of bacteria simply as plants, with genera and life history as in the case of other plants? In the first place, morphological characters are too ill-defined, if indeed they exist, and so he was forced to devise some other means of discrimination. This means was at first almost purely physiological. It largely depended upon the behavior of the individual sorts, or the groups of individual sorts, to certain nutritive processes. It depended upon what kind of media the bacteria grow upon, the temperature, and access to oxygen. Upon these physiological results in large part have been built our vast knowledge, economic very largely, of the many kinds of bacteria which are designated as species. But we may pass by the bacteria for a time and take up the larger organisms. I think the ones that are most useful in this connection are the strictly parasitic forms.

There is no more interesting and better understood group at the present time than the plant-rusts. The forms are sufficiently large to furnish a variety of species and genera exhibiting morphological characters, and yet, being strictly parasitic, are particularly restricted by the nature of the substratum. I will give one or two instances to illustrate. There is a group of forms—I will admit that I hardly know what terms .to use in speaking of them, but I will say a group of forms—which grow upon the various species of Carex. It is found that by taking rust spores from a single host of any particular Carex and sowing them upon an Aster, or a Solidago or an Erigeron, they will grow upon one of these genera, it makes not much difference what the species, but not upon the other two. Now if spores are taken from another Carex, the spores being so exactly like the former that they can not be distinguished by any visible characters, and sown upon plants of the same three genera, they may grow upon a different one than in the former instance, but not upon the remaining two. Thus finally we will get three sets of forms, one growing on Aster, one on Solidago and one on Erigeron, which can not be made to interchange, although they possess no evident morphological differences. This result depends, as I assume and as seems to be pretty well authenticated by all the researches, upon a question of nutrition. It does not depend upon any peculiar structure of Aster, Solidago or Erigeron; but here we have three genera which furnish different kinds of nutritive matter, or in some way differently affect the nutrition of the fungus, so that a fungus which has adapted itself to live upon one of these generic groups of plants will not live upon, or can not live upon, the other. It is a very nice adjustment to a particular kind of nutriment; I think we are quite well assured of that fact.

Now, the question arises, are these three species? If you choose to look up the literature, you will see that the speaker has described them as three species under three independent names. That does not mean that he believes they are truly three species, but that it was a convenient way of designating the three physiologically different kinds until a time when the matter could be more fully considered. Now, are these three species? Well, to confess to you, not to say it too loudly, I do not believe they are. They are not three species in the generally accepted sense because they can not be distinguished morphologically. What then is, or should be, our conception of a species? Is it something to be distinguished physiologically or morphologically, or in both ways? This is not a hypothetical question, because the literature of the subject that I represent is very full of descriptions of species based entirely or partially upon physiological data, and I am assured that we are not alone in this method of describing species.

If I understand aright, the ornithologists, at least some of them, are willing to assert that species may differ by characters which can not be described by words [laughter]; that is to say, you will know the species when you see it, but you can not describe it in any language by which another person may recognize it. I think that this is not a joke, but that it is really stated in earnest; because, I read it in *Science*. [Laughter.] But I fancy botanists have not gone quite that far.

Now, just another instance that arose during an extensive series of culture studies; that is the case of the Helianthus rusts. They have been described according to the host plants on which they grow. Each species of Helianthus and its close relatives appear to bear a distinct kind of rust, which acts in cultures as if it were an independent species. Yet any and all of these will grow on Helianthus annuus, a so-called bridging host. Are these different species different biological or physiological species, or simply forms or races? Possibly it would be well to refer them to some sub-category, as Dr. Britton has suggested.

I think when we come to study other parasitic forms thoroughly we shall find this condition equally true, for instance, in the genus Cuscuta among flowering plants. Furthermore it is probably true in many cases of plants which are not parasitic. We had an illustration this morning in the cultures of Penicillium exhibited by Dr. Thom, where characters of taxonomic importance were developed according to the medium on which the fungus was grown.

It seems highly probable that a physiological cause for variation, which may be considered as specific under many circumstances, could be traced throughout the vegetable kingdom. For purposes of study I assume that in most cases of physiological species, they may develop in the course of time into true taxonomic species, having distinct morphological characters, and that in cases such as I have enumerated, we are dealing with nascent species. The rules which have been laid down in the presentation of the subject by the previous speakers apply more particularly, I think, to forms well differentiated morphologically. But due recognition should be given to forms of physiological origin, as I have shown. Finally, I would say, at least it is a rule which I have formulated for my own guidance—it was a necessity that I should formulate some rule—that species, which are concepts, as I take it, for our convenience in discussing the various questions pertaining to plants, should be distinguished by sufficient morphological characters, the distinctions based upon physiological differences having subspecific rank. What constitute sufficient morphological characters must be left to the individual judgment.